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THE PROOF AND MEASUREMENT OF

ASSOCIATION

BETWEEN TWO THINKS.

By: C. Speaman,

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THE PROOF AND MEASUREMENT OF ASSOCIATION BETWEEN TWO THINGS

By C. Spearman

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INTRODUCTORY.

All knowledge - beyond that of bare isolated occurrence-deals with uniformities. Of the latter, some few have a claim to be considered absolute, such as mathematical implications and mechanical laws. But the vast majority are only partial; medicine does not teach that smallpox is inevitably escaped by vaccination, but that it is so generally; biology has not shown that all animals require organic food, but that nearly all do so; in daily life, a dark sky is no proof that it will rain, but merely a warning; even in morality, the sold categorical imperative alleged by Kant was the sinfulness of telling a lie, and few thinkers since have admitted so much as this to be valid universally. In psychology, more perhaps than in any other science, it is hard to find absolutely inflexible coincidences; occasionally, indeed, there appear uniformities sufficiently regular to be practically treated as laws, but infinitely the greater part of the observations hitherto recorded concern only more or less pronounced tendencies of one event or attribute to accompany snother.

Under these circumstances, one might well have expected that the evidential evaluation and precise mensuration of tendencies had long been the subject of exhaustive investigation BAR PROCE AND MEASUREMENT, OF ABBOOK ATION BUT DEAL TO BALL FOR

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Under there circonstances, one riset well is versited that the evidential evaluation and erected corsonables to tendere had long been the subject of example investigation

and now formed one of the earliest sections in a beginner's psychological course. Instead, we find only a general naive ignorance that there is anything about it requiring to be learnt. One after another, laborious series of experiments are executed and published with the purpose of demonstrating some connection between two events, wherein the otherwise learned psychologist reveals that his art of proving and measuring correspondence has not advanced beyond that of lay persons. The consequence has been that the significance of the experiments is not at all rightly understood, nor have any definite facts been elicited that may be either confirmed or refuted.

The present article is a commencement at attempting to remedy this deficiency of scientific correlation. With this view. it will be strictly confined to the needs of practical workers, and all theoretical mathematical demonstrations will be ommitted; it may, however, be said that the relations stated have already received a large amount of empirical verification. Great thanks are due from me to Professor Haussdorff and to Dr. G. Lipps. each of whom have supplied a useful theorem in polynomial probability; the former has also very kindly given valuable advice concerning the proof of the important formulae for elimi-

nation of "systematic deviations."

At the same time, and for the same reason, the meaning and working of the various formulae have been explained sufficiently, it is hoped, to render them readily usable even by those whose knowledge of mathematics is elementary. fundamental procedure is accompanied by simple imaginary examples, while the more advanced parts are illustrated by cases that have actually occurred in my personal experience. For more abundant and positive exemplification, the reader is requested to refer to the under cited research, which is entirely built upon the principles and mathematical relations here laid down.

In conclusion, the general value of the methodics recommended is emphasized by a brief critcism of the best correlational work hitherto made public, and also the important question is discussed as to the number of cases required for an experimental series.

Part 1. ELEMENTARY CORRELATION AND "ACCIDENTAL DEVIATION."

1. Requirements of a Good Method of Correlation.

Quantitative expression.

The most fundamental requisite is to be able to measure our observed correspondence by a plain numerical symbol. There

^{1. &}quot;General Intelligence" determined and measured, to appear in a subsequent number of this Journal.

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^{1.} Transal intelligences with rained une week week, to oweke in a cobec, were weaken of this economic.

is no reason whatever to be satisfied either with vague generalities such as "large," "medium, " small, " or, on the other hand, with complicated tables and compilations.

The first person to see the possibility of this immense advance seems to have been Galton, who, in 1886, writes: "the length of the arm is said to be correlated with that of the leg, because a person with a long arm has usually a long leg and conversely." He then proceeds to devise the required symbol in such a way that it conveniently ranges from 1, for perfect correspondence, to 0 for entire independence, and on again to -1 for perfect correspondence inversely. By this means, correlations became comparable with other ones found either in different objects or by different observers; they were at last capable of leading to further conclusions, speculative and practical; in a word, they now assumed a scientific character.

Mathematically, it is clear that innumerable other systems of values are equally concievable, similarly ranging from 1 to 0. One such, for instance, has been worked out and extensively used by myself (seeipp 15 ff). It therefore becomes necessary to discuss their relative merits.

(b) The significance of the quantity.

Galton's particular system is defined and most advantageously distinguished from all the others by the important property, that if any number of arms, for instance, be collected which are all any amount,)(a above the mean, then the corresponding legs will average rx; above the mean (with a middle or "quartile" deviation(2) of (1/1-r2); where (a - the quartile variation of the arms, (1 - that of the legs, and r is the measure of the correlation.

But another- theoretically far more valuable - property may conceivable attach to one among the possible systems of values expressing the correlation; this is, that a measure might be afforded of the hidden underlying cause of the variations. Suppose, for example, that A and B both derive their money from variable dividends and each gets 1/x th. of his total from some source common to both of them. Then evidently their respective incomes will have a certain tendency to rise and fall simultaneously; this correspondence will in any of the possible systems of values always be some function 1/x, but in only one of them will it actually be itself 6 1/x; in such a favored case, if A and B get, say, 20% of their respective incomes from the common source, the correlation between these two imcomes will also show itself as 0.20; and conversely, if A's income happens to be found correlated with that of B by 0.20, them

^{1.&}quot;Proceedings Royal Society of London", Vols XL and XLV.

^{2.} Commonly, but misleadingly, termed the "probable error."

is no reason shatover to be suffished sinher with vague go endlities such as 'leren' nedium, "smell," or, on the other hand, with couplinated tables and committated.

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Est another theoretically far more valuable - property, may conceivable attach to one smong the prestite systems of values expressing the correlation; this is, that a measure might be afforded of the bidder unlarilying cause of the variations. Suppose, for exable dividents undeach yets alm about the continuous from variable dividents undeach yets alm th. of his total from some source conton to both of them. Then evidently their respective incrues will alway correspondency to rise and fell simultaneously; this correspondence will in any of the possible systeme of values always as some function all, but in only one of them will it actually be itself ally; in such a favored case, if them will it actually be itself ally; in such a favored case, if common course, the correlation betasen these from the correction betasen these from the correction betasen these two incomes happens to be found cours atted with that of 3 by 0.30, thus happens to be found cours atted with that of 3 by 0.30, thus

^{1. &}quot;Proceedings Noyal costety of London", Vols N.B. and TLV.

^{2.} Comm wit, int midinalinedingly, term ! the probable error."

there is a likelihood that 0.20 of A's income coincides with 0.20 of B, leaving to either 0.80 disposable independently. The observed correlation thus becomes the direct expression of the relative amount of underlying influences tending for and against the correspondence.

In the above imagined instance, this desirable expressiveness belongs to the same above system of values proposed by Galton (and elabotated by Pearson). But this instance is exceptional and fundamentally different from the normal type. Evidently, A and B need not necessarily derive exactly the same proportion of their incomes from the common source; A might get his 0.20 while B got some totally different share; in which case, it will be found that the correlation is always the geometrical mean between the two shares. Let B be induced to put all his income into the common fund, then A need only put in 0.20 - 0.04, to maintain the same correlation as before; since the geometrical mean between 0.04 and 1 is equal to 0.20.

Now, in psychological, as in most other actual correspondences, A and B are not to be regarded as in the fixed bisection of our first case, but rather as in the labile inter-accommodation of our second case. Hence A, in order to be correlated with B by 1/x, must be considered to have only devoted 1/x2 (instead of 1/x) of his arrangement to this purpose and therefore still have for further arrangement 10-1/x2 which will enable an independent correlation to arise of \(\frac{1-1}{x} \) In short, not Galton's measure of correlation, but the square thereof, indicates the relative influence of the factors in A tending towards any observed correspondence as compared with the remaining components of A tending in other directions.

(o) Accuracy.

From this plurality of possible systems of values for the measure of the correlation must be carefully distinguished the variety of ways of calculating any one of them. These latter again, have various advantages and disadvantages, of which the principal is their respective degrees of liability to "accidental deviation."

For, though the correlation between two series of data is an absolute mathematical fact, yet its whole real value lies in our being able to assume a likelihood of further cases taking a similar direction; we want to consider our results as a truly representative sample. Any one at all accustomed to original investigation must be aware how frequently phenomena will group themselves in such a manner as to convincingly suggest the existence of some law - when still more prolonged experiment reveals that the observed uniformity was due to pure hazard and has no tendency whatever to further repeat itself.

Luckily, this one great source of fallacy can be adequately

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eliminated, owing to the fact that such accidental deviations are different in every individual case (hence are often called the "variable errors") and occur quite impartially in every direction according to the known laws of probability. The consequence is that they eventually more or less completely compensate one another, and thus finally present an approximately true result. Such elimination, however, must always remain theoretically imcomplete, since no amount of chance coincidence is absolutely impossible; but beyond certain limits it becomes so extremely unlikely that for practical purposes we can afford to neglect it. When a person loses 14 times run-ning at pitch-and-toss, he can reckon that such a series would not occur by mere accident one in 9,999 times, and consequently he will feel justified in attributing the coincidence to some constant disturbing influence. Similarly, to estimate the evidential value of any other observed uniformity, we only require to know how nearly the odds against chance coincidence. have approached to some such standard maximum as 9,999 to 17 But, as any standard must always be more or less arbitrarysome thinking it too lenient and others unnecessarily severeit is usual to employ a formula giving not the maximum but the middle deviation or "probable error". We may then easily find the probability of mere hazard from the following comparative table:

If the observed correlation divided by the phobable error be. __1 2 33 44 5 5 then the frequency of occur-

ence by mere hazard — 1/6 1/23 1/143 1/1250 1/19000 Now, the smallness of this probable error depends principally upon the number of cases observed, but also largely upon the mathematical method of correlation. Though a faultiness in the latter respect can theoretically be made good by increasing the range of the observations, yet such increase is not always possible, and, besides has other grave disadvantages which will be discussed later on. Other things being equal, therefore, the best method is that one which gives the least probable error For the benefit of the reader, this probable error should always be plainly stated; nothing more is required than a rough approximation; for while it is highly important to distinguish between a deduction worth, say, 0.9999 of perfect certainty and one worth only 0.75, it would be a mere splitting of straws to care whether a particular experiment works out to a validity of 0.84 or to one of 0.85.

(d) Ease of application.

^{1.} In the proper use of this expression.

eliminated, order to the feet "he' such seedental Povietions are different in every am ividual case fleroe are crien called the "vertable errors") and occur dathe "rpartially in every direction according to the brown last of probability. The consequence is that they evertually more or less commissing companience one enotion, and thus timelly present an suproximateria the result. Such elimination, base an elegan result the mutieslly im applete, either no smount of the nec ocincidence is altraighter of impossible; but havond certain lights it becomes to extragely amilarly that for restitation to recess we can afford to replict it. Then a norman case of times winming of mitch-and-form, he can readen dat grote a marie not outur by more arcident one in a,899 threa, and consequently be will feel justified in directive the coincidence to some constant distribing influence. Dimilarit, to estimete the evidential value of any other chaerves this another, we are seends, harios son as deminate as to sair virger won would of enimp bave andercached to some cuch stander hartman and f. 900 to 1. But, as any shaniand must always be more or less arbitrarysome thinging it too leadent and others unneresserily severait is usual to emply a formule wiring not the marture but the widdle deviation or "probacle ermon". A say them enally find the ero whiling of mere had thou the folicative connume-:alea: nvit

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I. in the proper use of this expression.

The most accurate ways of calculation are generally somewhat difficult and slow to apply; often, too there occur circumstances under which they cannot be used at all. Hence in addition to a standard method, which must be used for finally establishing the principal results, there is urgent need, also of auxiliary methods capable of being employed under the most varied conditions and with the utmost facility.

But here a word of warning appears not out of place. For such auxiliary methods are very numerous and their results, owing to accidnets, will diverge to some extent from one another; so that the unwary, "self-suggested" experimenter may often be led unconsciously - but none the less unfairly - to pick out the one most favorable for his particular point, and thereby confer upon his work an unequivocality to which it is by no means entitled. Any departure from the recognized standard methods are only legitimate, either when absolutely necessary, or for mere preliminary work, or for indicating comparatively unimportant relations.

2. Standard Methods Explained.

(a) Correlations between variables that can be measured

quantitatively.

This may be regarded as the normal type of correlation. Its standard method of calculation is that discovered by Bravais, in 1846, and shown by Pearson in 1896, to be the best possible. Pearson terms this method that of "Product moments."

The formula appears most conveniently expressed as follows:

$$\mathbf{r} = \frac{\mathbf{S}\mathbf{x}\mathbf{y}}{\sqrt{\mathbf{S}\mathbf{x}^2 \cdot \mathbf{S}\mathbf{y}^2}}$$

where x and y are the deviations of any pair of characteristics from their respective medians,

xy is the product of the above two values for any single in-

Sxy is the sum of such products for all the individuals,

Sx2 is the sum of the squares of all the various values of x,

Sy² is similarly for y

and r is the required correlation.

A simple example may make this method clearer. Suppose that it was desired to correlate acuteness of sight with that of hearing, and that for this puppose five persons were tested as to the greatest dostance at which they could read and hear a standard alphabet and sound respectively. Suppose the results to be;

^{1. &}quot;Memoires par divers savants" T, IX, Paris, pp255-332

^{2. &}quot;Phil. Trans., R.S., London" Vol.CLXXXVII, A, p.164.

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^{1. &}quot;Mammaras of alvers www.mis" i. i. Paris, mm256-352

^{\$.} Thil. Tras., ..., London' Vol.JLX TVII, &, p. 16+.

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so that $r = \sqrt{\frac{+12 - 12}{42 \times 25}} = 0$, and there, thus, is no correspondence, direct or inverse.

The "probable error" between any obtained correlation and the really existing correspondence has been determined by Pearson, as being "with sufficient accuracy" when a fairly large number of cases have been taken,

For discussion of correlation between characteristics whose distribution differs considerably from the normal probability curve as regards either "range" or "shewness," reference may be made to the works below. It may be remarked that the method of "product moments" is valid, whether or not the distribution follow the normal law of frequency, so long as the "regression" is linear.

(b) Correlation between characteristics that can not be meas-

ured quantitavely.

In the example quoted by Galton, of correspondence between the length of arm and that of leg, it may be noted that the correspondence is proportional quantitatively; a long arm has a tendency to be accompanied by a leg not only long, but long to the same degree. Now, in many cases, such proportionality is by the nature of things excluded; a printed work is possibly remembered better than one heard; but, hevertheless, we cannot in accordance with the preceding formula, ascertain whether degrees of visuality are correlated to retentiveness of memory, seeing that in the former case there do not exist any degrees, a word being simply either seen or not seen. Perhaps even

G. Lipps: "Die Theorie der Collectivgegenstände," Wundt's

Phil. Stud., Vol. XVII.

l. Udny Ytle: "Proc. R.S. London," Vol. LX, p477
Pearson: "Phil. Trans. R.S. London." Vol.CLXXXV, 1A,p71;
Vol CLXXXVI, 1 A, p343, and Vol. CXCI A, p.229.

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so that r = True - - 0. and there, thus, is no cor-

The "probable error" between or obtained correlation and the really extinting correspondent a last have note win a by Pearson, we peing with additional accurret when a fairliarge number of course as one calcon.

For discussion of correct of there teristies where distribution differs a maindrabily from the normal properties where curve as regards of there is not a false ness. The farmer of the may be made to the dure sector. It as a remarked that the method of "reduct sometime" is vold, reletier or not the distribution following as the frequency. The constant of the "regression" is the time.

(b) Correlation water cherecteristies that can not be desaured dushtitsvelt.

In the examine sucted or Galton, of correspondence hatreen the length of the length of the correspondence is encountinged outsities, it may be noted that the correspondence is encountinged outsities, of outsities, or has a tendence to be encountinged to the correspondence of the corres

more numerous are those cases where proportionality does indeed exist, but practically will not admit of being measured: for instance, it is probable that conscientiousness is to some extent a hereditary quality, jet we cannot well directly determine whether brothers tend to possess precisely the same amount of it, owing to the fact that we cannot exactly measure it.

In all such cases we must confine ourselves to counting the frequencies of coexistence. We can easily find out how often seen and spoken words are respectively remembered and forgotten. It has proved quite feasible to divide the children of a school generally into "conscientious" and "non-conscientious." and then to measure how much brothers tend to be in the same When we have proved this simple association, we may provisionally assume correlation of quantity also; that is to say, if the "conscientious," generally speaking, have a particular degree of tendency to pessess brothers likewise "con scientious," then boys with excessively tender scruples will have the same degree of tendency to possess brothers with similarly excessive tenderness, while those with only a moderate amount of virtue will be thus correlated with brothers also of only moderate virtue; further, the ethical resemblance may be expected to repeat itself in cousins, etc. only reduced in proportion as the kinship is diminished.

For measurement of this non-proportional association, a standard method, which may be termed that of "cross multiples." has been elaborated by Sheppard Bramley-Moore, Filon, Lee, and Pearson. The formula is, unfortunately, too long and complicated to be usefully quoted in this place. It will be found in the under cited work together with its probable error as determined by Pearson. In practice, it will generally have to be replaced by one of the more convenient methods to be next described.

3. Comparison by Rank.

This method of "cross multiples" is not only difficult and tedious of application, but also it gives a probable error nearly double that of "Product moments."

Now, it can often be altogether escaped in the case of quantities nnot admitted absolute measurement, by substituting instead comparison. This other way will be discussed at some length, as it has been largely used by myself and is believed chiefly responsible for some successful experiments. All characteristics may be collated from two quite distinct aspects: either (as in example of visual and auditory acuteness) by actual

^{1. &}quot;Phil. Trans." Vol. CXCII, A, p.141. 2. "Phil. Trans." Vol. CXCV A, pp.2-7

^{3.} Phil. Trans." Vol. CXCV A, 10-14.

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^{1. &}quot;FM11. Frens. ' Vol. OT. II. A. p.ld..

^{2.} Part. Trans. Tot. 03.07 . 57.7-

mensumation, or else by order of merit: we might say that a student, A, obtained 8,000 marks in an examination, while B only got 6,000; or instead, we might say that A was third out of 100 candidates, while B was only 20th. Precisely the same method of calculation may be again used in the latter case, simply substituting the inverse ranks, 97, 89, etc., for the performances, 8,000, 6,000, etc.

(a) Disadvantages of the "Rank" method.

In the first place, it may be objected that the observed cor-relation would then only hold good for persons of the same average differences from one another. For assuming, say, acute sight to be correlated with soute hearing; then the order of merit of A, B and C, as regards sight, is more likely to remain unaltered as regards hearing also, when the difference in their respective powers of vision is extremely marked, than when they are practically equal on the latter head. But the more numerous the persons experimented on, the less will be the average difference of faculty; it might, therefore, be supposed that the correlation would become continually less perfect as the experiments were made more extensive. This, however, would be a fallacy: 100 experimental subjects compared together by "Rank" would on the whole actually show appreciably the same average coffelation as 1,000, provided, that in either case the subjects are selected by chance; the amount of the correlation is not really dependent upon the difference between the grades, but upon the relation of this difference to the mean diviation; and noth of these increase together with the number of subjects; On the other hand, the correlation will undoubtedly diminish if the subjects be all chosen form a more homogeneous class; in a select training school for teachers, for example, general intelligence will throughout show smaller correlation with other qualities, than would be the case in a college for quite average young men of the same age; but this fact applies just as much to comparison by "Measurement."

The next possible objection is that comparison by rank bases itself upon an assumption that all the subjects differ form one another by the same amount, whereas A amy differ form B five times as much as B differs from C. But such an assumption would only take place, if correspondence by rank were considered to be wholly equivalent to that by measurement; no such assumption is made; the two aspects are recognized to be theoretically distinct, but advantage is taken of the fact that they give correlational values sencibly equivalent in amount. Even against the small existing discrepancy amy be set off a deviation of the same order of magnitude which is incurred when using measurement itself, owing to the practical necess-

ity of throwing the cases into a number of groups.

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The third and only solid objection is that rank affords a theoretically somewhat less full criterion of correspondence than does measurement; and the force, even of this argument, disappears on considering that the two methods give apprecibaly the same correlational values.

(b) Advantages of the "Rank" method.

The chief of these is the large reduction of the "accidental In normal frequency curve, the outlying exceptional cases are much more spaced apart than are those nearer to the average; hence, any accident disturbing the position of these exceptional cases will have unduly great effect on the general result of the correlation; and owing to this inequality in the influence of the errors, the latter will not compensate one another with the same readiness as usual. Moreover, it is just these hyper-influential extreme cases where there is most likelihood of accidental errors and where there very frequently prevails a law quite different from that governing the great bulk of the cases. As regards the quantity of this gain by using rank (abstracting from the last mentioned point, which cannot well be estimated in any general manner) there should be no difficulty in calculating it mathematically. siderable amount of empirical evidence, the probable error when using the method of "product mements" with rank appears to become less than two-thirds of that given by the same method with measurement, and therefore only about one-third of that given by the method of "cross multiples."

The next advantage is that rank eliminates any disparity between the two characteristics compared, as regards their general system of distribution; such a disparity is often not intrinsic or in any way relevant, but merely an effect of the particular manner of gaining the measurement. By means of rank, a series presenting the normal frequency curve can be compared on even terms with another series whose curve is entirely different. This cannot well be done when using measurements. (See p.7).

Rank has also the useful property of allowing any two series to be easily and fairly combined into a third composite one.

(c) Conclusion.

From the practical point of view, it is so urgently desirable to obtain the smallest probable error with a given number of subjects, that the method of rank must often have the preference even when we are dealing with two series of measurements properly comparable with one another.

Theoretically, rank is at any rate preferable to such a hybrid and unmeaning correlation as that between essential measurements on the one side and mere arbitrary classification on the other. As the latter occur in most psychological correlations,

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the only other resource would be to avoid measurements altogether by using the method of "cross multiple." But this trebles the size of the probable error, and therefore renders it necessary that the subjects should be no less than nine, times as numerous; such an enormous increase, even if possible, would generally be accompanied by disadvantages infinitely outweighing the supposed theoretical superiority of method.

The above advantages are still further enhanced whenever dealing with one-sided frequency curves, such as are furnished by most mental tests. For in these cases the great bulk of influence upon the resulting correlation is derived exclusively from the very worst performances and is consequently of a

specially doubtful validity.

In short, correlation by rank, in most cases a desirable procedure, is for short series quite indispensible, rendering them of equal evidential value to much longer once treated by other ways. Luckily, it is precisely in short series that gradation by rank is practically attainable.

(4) Auxiliary Methods.

These, as has been said, are only for use when there is adequate reason for not employing the above "standard" methods. Any number are devisable. Their resulting correlational values do not quite coincide with those found by the standard says, but nearly enough so for most practical purposes.

(a) Auxiliary methods of Pearson
Several very ingenious and convenient ones are furnished by
him, but all of similar type and requiring the same data as
that of "cross-multiple." They are therefore for use when
the compared events do not admit of direct quantitative correlation. The following appears to combine facility and precision
to the greatest degree:

$$r = \sin \frac{\pi}{2} \frac{\sqrt{ad - \sqrt{bc}}}{\sqrt{ad + \sqrt{bc}}}$$

where the two compared series of characteristics, say P and Q are each divided into two (preferably about equal) classes; if the case is one where quantity exists but cannot be absolutely measured, P II will comprise the instances in which P is in manifest deficiency; but if the compared characteristics essentially exclude quantity, P II becomes the instances where P is

^{1. &}quot;Phia. Trans. R.S.L.," Vol. CXCV, A, pp 1 and 79.

^{2.} They are all refinements of the original formula, r - ad + bo published by Yule, Proc. R.S.L., Vol.LXVI, p.23.

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(4) A wiliary Mathods.

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absent; similarly Q. Then,

If a + b is not very unequal to c + d, the probable error may be taken at about $1.1/\sqrt{n}$, where n — the number of instances in the whole of P or of Q.1

Returning to our previous illustration, suppose that it was desired positively to ascertain the merits of instruction by writing and by word of mouth respectively. Ten series, each consisting of ten printed words, have been successively shown to a class of twenty children, who each time had to write down by memory as many as they could. The experiment was next repeated, but reading the words aloud instead of showing them. Of the 2,000 visual impressions 900 were corectly remembered, while of the same number of auditory ones only 700 were retained.

then a _ 900, b _ 700, c _ 1,100, d _ 1,300, and

$$r = \sin \frac{\pi}{2} \frac{\sqrt{900} \times \sqrt{1,300} + \sqrt{700} \times \sqrt{1,100}}{\sqrt{900} \sqrt{1,300} + \sqrt{700} \sqrt{1,100}} = 0.16$$

the probable error then comes to $1.1/\frac{4.000}{4.000}$ - nearly 0.02, or about 1/8 of the above correlation; so that the latter would not occur by mere chance once in 100,00 times.

We thus see that there is at any rate good prima facie evidence of some superiority on the part of the visual sense. Also, if the experiment has been fairly executed and adequately described, any subsequent verification under sufficiently similar conditions, by other experimenters, should result in a concordant correlation, probably between 0.04 and 0.28.

Moreover, we have obtained a direct estimate of the importance of this apparent superiority of the visual sense; for the square of the correlation amounts to 0.025; so that of the various causes here tending to make the children remember some words better than others, the difference of sense impressed comes to about one fortieth part (see p. 4).

1. More accurately,
$$\sin 0.1686 \pi (1 - r^2) \sqrt{\frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \frac{1}{d}}$$

ebsent; wimilarly . These

If a # bis cot V ry are $v_0 = v_0 + v_1 = v_1 + v_2 = v_3$ and the man be taken as about $1.1/\sqrt{2}$. There is $\frac{1}{2} = v_1 = v_2 = v_3 = v_1 = v_2 = v_3 = v_3 = v_4 = v_3 = v_4 = v_4 = v_4 = v_5 =$

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ther e = 900, b = 700. c = 1,000, c = 1,000. cr

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(b) Method of proportional changes.

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This is very often convenient, being especially applicable to a large number of psychological experiments, and so easy that the result can be approximately seen on inspection. Here,

result can be approximately see:
$$r = \frac{3}{2} \cdot \frac{a - b}{a - b}$$

where a - the number of cases that have changed in accordance with the supposed correspondence, and b - the number that have changed in contradiction of it. The probable error again comes to 1.1

Suppose, for example, we were demonstrating that intellecttal fatigue may be satisfactorily investigated by the method of Griessbach. 2 With this view, we have applied his test to 100 boys before and after their lessons. In the latter case 68 of them have presented the expected duller sensitivity, but 32, on the contrary, have shown a finer discrimination than before work.

Now, clearly, had the correspondence been perfect, all the hundred would hve become worse. Thus,

$$\frac{3}{r} = \frac{68 - 32}{100} = 0.54.$$

As the probable error comes to 0.11, our imaginary correlation is five times greater, and therefore would not have occurred by mere accident more than once in 1,250 times; so that we become practically certain that the sensitivity of the skin really does measure fatigue.

It now becomes easy to compare the quantity of this fatigue at different stages of work. Let us say that further experiments, after lessons lasting one hour longer than before, showed the correlation had risen to 0.77. Thereby we see that the influence of fatigue swells from 0.542 to 0.77, that is, from being 1/5 to being 3/5 of all the sources of veriation in cutaneous sensitivity. Such a result has a very different scientific significance from, say, any conclusion that the average sensory threshold had enlarged by so many more millimetres.

3. Againing that is to say that all the hors become fatigued by

^{1.} Hence, when the correlation is very complete, say over 0.75 the above formula gives appreciably too large values; as the amount reaches 0.90 and 1, the first factor must be reduced from 3/2 to 5/4 and 1 respectively.

^{2.} This, as is well known, consists in determining the least distance apart at which two points of contact can be distinguished as being double and not single.

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where s - the number of cases the coverence of a sordence with the surpaged dorse on encount that they the property that they there are not solve one of the comestion of the comest to 1.1

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As the probable error cases so 0.11, or instinent error - tred - tion is fire times growter, and therefore a cit of case of tered by mare rocident more time time time time to 1.350 finally or that o become apposize the carbain that the sencialists of the carbain that the sencialists of the carbain of the carbain.

It now becomes carry to compare the court of the ference of different stages of norm. The use of the ference of the large of the large

^{1.} Hence, when the correlation for vary or site, or law the following the speve form leading the correlation of the specific state of the substitution of the context of the correlation of the context of the correlation of

^{8.} This, so is well known, constanting the factor of the distance apart at about two police of our test of a substance and a factor of contract and as infigure.

B. Assuming, that is to say, thet will the rejer some froigned by

Moreover, our test can be easily and precisely compared with any of the various other recommended procedures, being more reliable than all which present smaller correlations and vice versa.

(c) Method of class averages.

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It often happens that measurements (or ranks) are known but not in such a way as to be able to use either the method of "product moments" or even any of the methods of Pearson. Under such circumstances, I have found it very useful to be able to apply the following relation:

r <u>- d</u>

where d is the observed difference between the average measurement (or rank) of the P's accompanied by Q I and that of those accompanied by Q II, and D is the greatest difference that was possible (such as would have occurred, had the correspondence been perfect). If Q has been divided into two about equal portions, D will be equal to twice the middle or "quartile" deviation from the average in the whole series P; while if Q has been divided after the usual fashion into three such portions, only the two outer ones can be used and then D = 2.87 times the above middle deviation (again taken in the whole series P)

Suppose, for example, that we wish to ascertain whether the well known test of "reaction-time" gives any indication as to the person's general speed of movement. We try a hundred persons both in reaction-time and in speed of running 50 yards. Then we divide the reaction-time records into two classes, I containing all the quickest performers and II al the slowest. We now see how long these two classes of reacters took respectively to run the fifty yards, and what was the middle deviation from the everage among all the runners taken together. Let us put the average of class I at 6 seconds, that of class II at 6.5 seconds, and the general middle deviation at 1.1 seconds. Then

$$r = \frac{6.5 - 6}{2 \times 1.1} = 0.23$$

The evidential value of the result is given approximately, even for small values of n, by the following relation:

Probable error
$$-\frac{1.17}{\sqrt{n}}$$
 $\sqrt{\frac{n}{n}+\frac{1}{2}}$

where n is the total number of cases considered. In the three-

Norcever, our test own or easily the radiosity of part and any of the various other recommended and and areas being one reliable than ell which pare will and a correlations and ware verse.

(o) Method of class svoruses.

It offen harpens thet measurements i or rado, no harva but not in such a may or to be the nera cither industration of "product mements' or even only of the materia of on yor. Under such direconstance. I have found it wany neight to sa ship to apply the following relevies:

where d is the observed difference when the restriction of uremont (or rest) of the P's sector emiss by all and that of these accompanies by all and the P's sector emiss by all and there there there is no difference that was possible (such a again) are precised of the object of the or 'quartile' deviation in the verse is the belief of the while if d has been divided after the object of the while if d has been divided after the object of the such of the object of the object

Suppose, for exemple, ther we will be used to wistly the well known test of reserion-time? rives any indication as to the persons both in resetion-time and in speed of running To acta. Phan we divide the resetion-time and in speed of running To acta. Than we divide the resetion-time recepts records near two classes. I containing all the cudekest verformers and II at the slivist. We now see how long whose incomers and II at the slivist tively to run the fifty yerds, and that was the mindle falled from from the everye among all the runners we at the mindle falled from the everye of class I at a seconds, and the seconds. Art of the seconds, and the seconds. In the runners we are the seconds.

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The syidential value of the result is given approximately, even for small values of n. by the last of relations

where i is the tried number of mees the ergs. In the charge

fold instead of twofold division, the probable error becomes nearly

 $\frac{1.4}{\sqrt{n}} \qquad \frac{\sqrt{n-1}}{\sqrt{n-2}}$

In the above instance, we find that the observed correlation is little over double the probable error; as so much would turn up about once in six times by mere accident, the evidence is not at all conclusive. Therefore we must either observe many more cases - 600 would be necessary to reduce the probable error to 1/5th of the correlation - or else we must fond a better method of calculation. If rank had been employed instead of measurement, the evidence would already have been fairly good, and could have been put beyond all reproach by the addition of another 150 observations. If rank had been employed in conjunction with the methof of "product mements" or that of "rank differences," the required smallness of probable error could have been obtained by as few as 36 cases in all!

The method of "class averages" is especially valuable in deciphering the results of other investigators, where the average performances and the middle deviations are usually given (in good work), but not the data required for any of the other methods.

(d) Method of rank differences.

This method appears to deserve mention also, seeing that it seems to unite the facility of the auxiliary methods with a maximum accuracy like that given by "product moments". It depends upon noting how much each individual's rank in the one faculty differs from his rank in the other one; evadently this will be nil when the correlation is perfect, and will increase as the correlation diminishes.

1. This general idea seems to have been first due to Binet and Henri ("La fatigue intellectuelle" p.252-261), who, however do not work it out far enough to obtain any definite measure of correlation. Accordingly, Binet makes little further attempt in later research (L'annee psychologique, Vol.IV) to render it of service, and soon appears to have altogether

dropped it (L'annee psychologique, Vol. VI.).

The same isea occurred to myself and was developed as above, without being at the time acquainted with the previous work in this direction by Binet and Henri. In obtaining the above formulae I was greatly assisted by Dr. G. Lipps' showing generally that when an urn contains n balls numbered 1,2,3,..n, respectively; and when they are all drawn in turn (without being replaced); and when the difference is each time noted between the number on the ball and the order of its drawing; then the most probable (or middle) total sum of such differences, added together without regard to sign, will be

 $\frac{-}{2} \frac{n^2 - 1}{3}$

Previously I had only calculated this value for each particular size of n required by myself. Prof. Hausdorff further showed, generally, that such sum of differences will present a mean square deviation (from the above most probable value)

 $-\sqrt{\frac{(n+1)(2n^2+7)}{45}}$

fold instead of twofold division, the urberland order brownss meerin

In the above instance, we find a. . The deems - normalist a is little over double the probable arror; as a must would tur up about case in six sines ip hous necitable. Sin artimus is not st ell conclusive. Tabarètere we munt estrer cherre amn; more esse - 670 woult be nicepsent to reduce or archéble error to 1/6th of the correspion - or dee is du t fent a bather mothed of daleulation. If your had beer amployed fratesd of measur oment, the avidence would alice of like i been the trip good, and could have been put, beyon't it or so, big the addition of another lot newerry teams. If the both a companies of the played in conjugation like when we will be to the conjugation of the when we will be to the conjugation of the con -woods i smerilene bashows sit ".asonerollib What" to Jadi to ble strar could have been abtained by activity to the could

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(i) Mathod of reak the erances. This method appears to deserve meaning where, weeking the it seems to white the footlity of the savilians of ode with the maximum services like thet fiven by 'paot' a neermee'.

It depends upon noting how much ever individual a mank in the end iwealty differs from his mod in the other end; evidence; this will be nil when the ocreplation is erricet, and oill increase as ti - correlation fisibidacs.

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BE TOTALINE, BING but he rever of herrouse week comes and न्यक्षा अर्थ में मिल्ल के विवेद जर्मा कर विवेद कर्मा कर प्रविद्या कर स्थापित के मिल्लिक क्षेत्र कर कर कर कर कर ions work in this direction ht dines and deart. 'a o'taining the above formular 1 and pro 10 - 19:00 - 17. C. Clapet showing generally that when as one orchebe. Darsdon enti are gon't a bne :ylavitachem .c. . N.S. I in turn (without seing replace); and chem the liften rediscount tire moved between the analysis of the color time. rib. to so) olds or a lent ent outpose a the commo total sun of sach of Frenchis, third to otter although regard to tipr. This of

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Previously I be a only esled. I subject the rest property cultr site of myre tired by spacif. For Worlf Turbest showed, ser rally. That man, and directores will arean a tier olderen de mayer, est mont est sivob anauja naam a The relation is as follows:

$$R = 1 - \frac{350}{n^2 - 1}$$

In it, ho ; where Sd is the sum of the differences of rank for all the indi-

n is the total number of individuals,

and R is the required correlation.

The probable error will then be approximately, even for

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small values of n, $-0.4/\sqrt{n}$. To take again the example from p.9, we number the five persons according to their order of merit in hearing and seeing respectively.

person	seeing rank	hearing rank	difference
· • • • • • • • • • • • • • • • • • • •	1	·] 1	0
B	. 2	4	2 .
- 4 40 . C 1.	3	5	2
the star D	4	115.3	. 1
E E	5	. 2	13
		r	****
		76	Sd - 8
/ - / - /			- ,

THE DIST

L. E S Y FORING.

and again we find that there is no correlation, direct or inverse. This method, though very accurate and pre-eminently quick in application, has unfortunately four serious disadvantages. It can be only used for ranks, and not immediately for measurements.

The probable error given is only that showing how great correlations may be expected from pure accident when there is no really existing correspondence between the two characteristics. It does not (like Pearson's probable error for the method of "product moments") directly show how much the observed correlation may be expected to differ by accident from any correspondence. that does exist.

The various possible values of Sd are found to fall into a frequency curve of marked asymmetry; so that we cannot (as in all the other methods here given) take the minus values of R as representing so much inverse correlation. This defect could be remedied mathematically; but there are also other respects in which this side of the frequency curve appears unsuitable for our purpose, so that it is better to treat every cor-

^{1.} This formula becomes slightly incorrect, whenever two or more individuals are bracketed as having precisely the same rank; but the consequent error is usually to be worth considering.

The relation is sa follows:

bhare Gd is the sur of the differences of real the indi-

: n is the tote muchen of individuels,

and R is the required occasintion.

The probable error will then be a suroximately, even for

small values of n. - 0.-/in.

To take again the eramble from n.J. sa number the vive persons according to their order of wordt in hearth of resing respectively.

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and again we find that there is no carrelation, direct or inverse. This method, though very accurate and pro-semimently quick in application, has unfortunately four sarious dissipantance.

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The various possible velues of a are loads to fall into a fractionary curve of saried asymmetry; a cat we carnot (sain in all the other methods here given wate he sinat values of R as representing so made inverse after. I'il a after could be remedied mathematically; but there we are the other respects in shich this side of the frequency curve area. The unstable for or aurouse, so the side of the solution to treat events unspects in shich this side of the frequency curve unsputtable for or aurouse, so that it is better to treat event corr

^{1.} This formula becomes alightly in thest, whenever two of more individuals are brill our the consequent error is usually to be eastful considering.

relation as positive (which can always be done by, if necessary,

inverteng the order of one of the series). Finally, this value R is not numerically equivalent to the "r" found by all the other methods, but for chance distributions appears - \(\text{r} \). So far, the proof of this relation is only empirical, but it respectively on a large number of cases taken, however, only between 0.20 and 0.60. If it be accepted r can at once be found from the following table; R 0.05 0.10 0.20 0.30 0.40 0.50 0.60 0.70 0.80 0.90 1 r 0.13 0.22 0.34 0.44 0.54 0.63 0.71 0.79 0.86 0.93 1

Part II. Correction of "Systematic Deviations."

1. Systematic Deviations Generally.

In the first part, we have seen that any correlational experiments however extensive, can only be regarded as a sample" out of the immense reality, and will consequently present a certain amount of accidental deviation from the real general tendency; we have further seen that this accidental deviation is measurable by the "probable error" whose determination, therefore becomes an indispensab; e requisite to all serious research.

But now we are in danger of falling from Sculla into Charybdis. For after laboriously compiling sufficient cases and conscientiously determining the probale error, there exists a very human tendency to cease from labor and inwardly rejoice at having thus risen from common fallacious argument to the serene certainty of mathematics. But whether or not such complacency may be justifiable in pure statistical inquiry, it is at any rate altogether premature in the kind of research that we are at present contemplating; we are not dealing with statistics, but with a line of work so fundamentally different that it may be aptly distinguished by the term of "statisticoids." Here the accidental deviation is not the sole one, nor even the most momentous; there are many other enemies who are unmoved by the most formidable array of figures. These consist in such deviations as, instead of merely being balanced imperperfectly, lie wholly pm the one side or the other. As in ordinary measurements, so too in correlation, we may speak, not only of "accidental" " variable" or "compensating" inaccuracies, but also of "systematic, 2" constant," or "non-compensating" ones.

These systematic deviations are of very varied nature, the post insidious being as usual self suggestion. To take, for instance, one of our recent examples, suppose that we have applied the Griessbach test to a number of children before and relation as positive (waich can be due of the edition in the parting to a order of one of the edition of the ed

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after their lessons, and have found the desired correlation between fatigue and cutanuous insensitivity it still remains exceedingly difficult to convince ourselves that we executed our tests entirely without favor or affection; for it is almost impossible to determine a series of sensory thresholds without some general tendency, either to bring them towards the desired shape, or else - endeavoring to escape such bias- to force them in the opposite direction. To convince others of our impartiality may be harder still. Even this sort of deviation is to be remedied by our proposed exact method of provided ure for by it we obtain perfectly definite results which any impartial experimenters may positively corroborate or refute.

2. "Attenuation" by Errors.

From page 3 it will be obvious that a correlation does not simply depend on the amount of concording factors in the two compared series, but solely on the propertion between these concording elements on the one hand and the discording ones on the other. In our examplem it did not matter whether A and B each had one punned or a thousand pounds in the common funds but only whether the amount was a small or large fraction of their whole incomes. If the discordance, 1-x, be nil, then the concordance x is thereby perfect, that is, - 1; and if the influence of the discordant elements be sufficiently increased, then any concordance will eventually become infinitely small.

To consider a still more concrete example suppose three balls to be rolled along a well-kept lawn; then the various distances they go will be almost perfectly correlated to the various forces with which they were impelled. But let these balls be cast with the same inequalities of force down a rough mountain side; then the respective distances eventually attained will have but faint correspondence to the respective original

nomenta.

Thus it will be clear that here the accidental deviations have a new consequence simultaneous with, but quite distinct from, that discussed in the last chapter. For there, they impartially augmented and diminished the correlation, tending in a prolonged series to always more and more perfectly counterbalance one another; and in ordinary measurements, this is their sole result. But here in correlations, they also have this new effect which is always in the direction of "attenuating" the apparent correspondence and whose amount, depending solely on the size of the middle error, cannot be in the least eliminated by

I. This fact has already been mathematically expressed in the last chapter by the value of correlation between two series being proportional (inversely) to the value of the middle deviation inside the series (see p. 15).

after their lessons, and have found the derired correlation between fatigue and obtained breakingly difficult to convince ourselves that we executed our tests ortively without favor or aftential; for it is almost tests ortively without favor or aftential; for it is almost test in the to determine a remiss of researcy thresholds without some general tendency, without to bring them towards the desired shape, or also - indominate to ascape ruch liter to force them in the opposite direction. To convince others of our impartially may be under still. I wential of deviate to the course for by the out in perfectly definite results which are codure for by the out in perfectly definite results which are any increased or or oborate or refute.

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To consider a still more concrete on it. ampphes three balls to be rolled along: all-kept lawn; then it.e various distinces the go, will be almost perfectly constituted to the sations forces with which they were impelied. For the tot that balls as cast with the same inequalities of force down a rough rountial size; then the respective distances on humally attained will have but faint correspondence to the respective original moderte:

Thus it will be clear that here the sectionated devistions have a new consequence of multimentally, but outle distinct from that discussed in the last chapter. In the last chapter, they importially away realed and distributed and distributed and distributed and distributed and distributed and series to slways now and none enricetly counterbalance one another; and in or itary mess monerate, dis is their role of result. But here in corrections, is your mayor his residentalist always in the direction of "attenuating" the operation of attenuating the operation of the depending olear on the size of the direction of the last olears of the direction of the last olears of the direction of the last olears of the direction of an outle of the direction of the last of the direction of an outle of the direction of the direc

I. Tair is that elementy been metaermain the expressed in the least chartent of the least charten by the reluc of correlation entrest two series leing proportional time enact; to the volue of the middle evistion increasing the remies that the continue of the middle evistable.

any prolongation of the series. The deviation has thus become

general or "aystematic."

37.61

Now suposse that we wish to ascertain the correspondence between a series of values, p, and another series, q. By practival observation we evidently do not obtain the true objective values, p and q, but only approximations which we will call p and q'. Obviously, p'is loss closely connected with q' than is p with q. for the first pair only correspond at all by the inter-mediation of the second pair; the real correspondence between the p and q shortly rpq, has been "attenuated" into rptq;

To ascertain the amount of this attenuation and thereby discover the true correlation it appears necessary to make two or more independent series of observations of both p and q. Then

DI CHEST where p'q' - the mean of the correlations between each series of values obtained for p with each series obtained for q.

- the average correlation between and ahother of these several independently obtained series of values for p. .

the same as regards q.

and trop in the required real correlation between the true i i.e. objective values of p and q.

Thus, if for each characteristic two such independent series of observations be made, say pipeqi and qe then the true - 41. 1.7 Dr. . . .

Should circumstances happen to render say pl much more. accurate than p2 then the correlations involving pl will be considerably greater than those involving p2. In such case,

the numerator of the above fraction must be formed by the geometrical instead of by the arithmetical mean; her by the accidental errors of the respective observations cease to eliminate one another and therefore double their final influence; they also introduce an undue diminution of the fraction.

In some exceptional and principally very theoretical cases, it may happen that either of the actual measurements, say p'l is

By an inversion of the above formula, the correlation between two series of oberavations will be found a useful measure of the accuracy of the observation.

any prolongation of the corles. The deviate of this papers

Now supen as that we wish to ascertain the course pondence between a serios of valued, p, and ancerent actions, . By practive tival observation so evidently for not obtain the true objective values, p and q, but only approvinationary all the clin of obviously, p' is less that and q'. Obviously, p' is less that the contraction of the first pair and a section of the first pair of the contraction of the section of the contraction of the section of the sec

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the numerator of the rower freeding and the common by the geometrical alignment of by the clithen bireless and action by the accidental common of the respective opening the standard constant of the respective and the respective double that it is a full constant also introduce an added distinution of the fraction.

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connected with q'(or q) quite independently of p or any other link common to p'2. Then, the correlation rp'q' will be to that extent increased without any proportional increase in rp'p';

hence our above formula will fallaciously present too large a value.

A greater practical diffuculty is that of obtaining two series sufficiently independent of one another. For many errors are likely to repeat themselves; even two separate observers are generally, to some extent warped by the same influences; we are all imposed on by, not only the "Idola Specus," but also the "Idola Tribus" and the "Idola Fori. "In such Sase, the above formula is still valid, only, its correction does not go quite far enough, - a fallacy at any rate on the right side.

An actual instance will best show the urgent necessity of correcting this attenuation. In a correlation between two events, say P and Q, I obtained three independent observations both of P and of Q. The average correlation for those of P with those for Q was 0.38 (= rp!q'); the average correlation of those for P with one another was 0.58 (= rp!p'), the same for Q was 0.22 (= rq!q'). Therefore, the correspondence

between the real events, P and Q, comes by reckoning to

6.58 X 0.22 approximately 1; so that the correspondence

instead of being merely 0.38, appeared to be absolute and complete.

Attenuation by errors can also be corrected in another manmer, which has the great advantage of an independent empirical
basis, and therefore of not being subject to either of the two
above mentioned fallacies besetting the other method. Hence,
when the results coincide both ways, the fallacies in question
may thereby be considered as disproved, for it is very unlikely
that they should noth be present and in such proportions as to
exactly games one another. In this method, instead of directly

employing the values $p_1p_2p_3$, etc, we amalgamate them into a single list; by this reansive clearly deliminate some portion of the individual observational errors, and thereby we cause any really existing correspondence to reveal itself in greater completeness. Now, this increase in correlation from this partial elimination of errors will furnish a measure of the increase to be expected from an entire elimination of errors. Assuming the mean error to be inversely proportional both to this increase in the correlation and to the square root of the number of lists amalgamated, the relation will be:

connected with $q'(or_A)$ quite independently of p or any other link common to p'E. Then, the correstion $r_p'q^*$ will be to that extent increased without any proportional increase in $r_p'p^*$;

hence our above formula will fallacious; present too large a value.

A greater practical diffuculty is that of obtaining two series sufficiently independent of one another. For many approve are likely to repeat themselves; even two heparate observers gre generally, to some extent warped by the same influences; we are all imposed on by, not only the "Idola Special," but alsothe "Idola Tribus" and the "Idola Fort!" in such Sacer the above formula is still valid, only, its correction does not go oute for enough. - a fallacy at asy sate on the right cide.

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Attenuation by errors can also corrected in another manmer, which has the great advantage of an independent empirical basis, and therefore of not being subject to either of the two above mentioned fallaries besetting the other rethod. Hence, when the results coincide both ways, the fallseies in question may thereby be considered as disproved, for it is very ullively that they should noth so present and in such proportions as to exactly gamget and another. In this method, instead of direct speaks

employing the values Pipp P2, etc, we amign mate them into a single list; by this radner medical density which individual observational errors, and thereby we can carry really existing correspondence to reveal itself in greater completeness. Now, this increase in correlation from this pertial elimination of errors will furnish a measure of the increase to be expected from an entire elimination of errors. Assuming the mean error to be inversely proportional both to this increase in the correlation and to the square root of the unity orease in the correlation and to the square root of the unity of lists and lester analysmated, the relation will be:

connected with q^* (or q) quite independently of p or any other link common to p_2 . Then, the correlation $r_{p'q'}$ will be to that extent increased without any proportional increase in $r_{p'p'}$; hence our above formula will fallectously present too large a value.

A greater practical difficulty is that of obtaining two series sufficiently independent of one another. For many errors are likely to repeat themselves; even two separate observers are generally, to some extent, warped by the same influences; we are all imposed on by, not only the "Idola Specus," but also the "Idola Tribus" and the "Idola Fori." In sah cases, the abobe formula is still valid, only its correction does not go quite far enough, - a fallacy at any rate on the right side.

An actual instance will best show the urgent necessity of correcting this attenuation. In a correlation between two events, say P and Q. I obtained three independent observations both of P and of Q. The average correlation for those of P with those for Q was 0.38 (- rp'q'); the average correlation of those for P with one another was 0.58 (- rp'p'); the same for Q was 0.22 (- rq'q'). Therefore, the correspondence between the real events, P and Q comes by reckoning to

approximately 1; so the the correspondence

instead of being merely 0.38, appeared to be absolute and complete.

Attenuation by errors can also be corrected in another manner, which has the great advantage of an independent empirical basis, and therefore of not being subject to either of the two above mentioned fallacies besetting the other method. Hence, when the results coincide both ways, the fallacies in question may thereby be considered as disproved, for it is very unlikely that they should both be present and in such proportions as to exactly cancel one another. In this method, instead of directly employing the values p₁ p₂ p₃, etc. we amalgamate them into a single list; by this means we clearly eliminate some portion of the individual observational errors, and thereby we cause any really existing correspondence to reveal itself in greater completeness. Now, this increase in correlation from this partial elimination or errors will furnish a measure of the increase to be expected from an entire elimination of errors. Assuming the mean error to be inversely proportional both to this increase in the correlation and to the square root of the number of lists amalgamated, the relation will be:

connected with 4! (or 4) quite independently of p or any oract link common to y_2^{\dagger} Them, the correlation $r_{p'q'}^{\dagger}$ will be to that extent increased without any proportional increase in $r_p^{\dagger} r_{p'}^{\dagger}$ hence our above formula will fallsciously precent too large a value.

A greater pactical difficulty is that of outsining two series sufficiently independent of one another. For many errors are likely to repeat themselves; even two separate observers are generally, to some extent, warped by the same influences; we are all imposed on by, not only the "Idola Specus," but also the "Idola Tribus" andthe "Idola Wori." in sen cases, the above formula is still valid, only its carection does not appoint far enough, a fallacy at any mate on the right side.

An actual instance will best show the urgent necessity of correcting this attenuation. In a correction between two events, say P and Q, I obtaind three independent observations both of P and of Q. The average crivelation for those of P and of Q. The average crivelation for those of P and of those for Q was 0.38 (_ rp'q'); the average correlation for those for P with one another was 0.58 (_ rp'p'); the same for Q was 0.22 (_ r,''). Therefore, the correspondence to between the real events, P and Q comes by reckening to

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exactly concel one another, in this method instead of directly
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of the individual observational errors, and thereby we exuat any really existing correspondence to revent itself in greature completeness. Now, this investes in correlation from this partial elimination or errors will furnish a measure of the increase to be expected from an entire elimination of errors. Assuming the mean error to be inversely proportional both to this increase in the correlation and to the square root of the number of lists amalgarated, the relation will be:



where m and n - the number of independent gradings for p and q respectively,

gradings for p and those for q,

and rp"q" = the correlation of the amalgamated series for p with the amalgamated series for q.

In the above quoted instance, the three observations for series P were amalgamated into a single list, and similarly those for series Q. Upon this being done, the two amalgamated lists now presented a correlation with one another of no less than 0.66(= rpm m). Thus by this mode of reckoning, the real correspondence became

 $\frac{4}{3\times3\times0.66-0.38} = \text{once more approximately 1,}$

so that this way also the correspondence advanced from 0.38 to

absolute completeness.

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If more than two independent series of observations are available, we may acquire additional evience by trying the effect of partial amalgamtion. Instead of throwing all our obtained values together, we may form a set of smaller combinations for each of the two compared characteristics, and then see the mean correlation between one set and the other. In our obove instance instead of summarily considering p'1 p'2 p'3, we can have pi pi pi and pi pi and find out their mean correlation with similar values for q. This works out actually to 0.55. Hence

actually to 0.55. Hence $\frac{4}{2 \times 2 \times 0.55} = 0.38 = \text{approximately 1.}$

Thus again, by this third may, where both terms are the mean of 9 observed correlational values, the correspondence once more rises from the apparent 0.38 to the real 1. (1)

3. Limits of Associative Problems.
We have seen that "the length of the arm is said to be correlated with that of the leg, because a person with a long arm has usually a long leg and conversely;" also that this correlation is defined mathematically by any constant which deter-

(1) The exactness of the coincidence between the two methods of correction is in the above instance neither greater nor less than generally coours in practics. It was singled out, in order to show that the formulae still hold perfectly good even for such an enormous rise as from 0.38 to 1. The posibility of such a rise is due to the unusual conditions of the experiment in question, whereby the three observations of the same objective series presented the extraordinarily small intercorrelation of 0.22.

where a and n - the number of independent gradings for p and a respectively,

rotat = the mean correlation between the various gradings for p and those for u.

the correlation of the amalgamated series for b with the amalgameted series for u.

In the above quoted instance, the three observations for series P were amalgameted into a single liet, and similarly those for series J. Upon this being done, the two amalgansted lists now presented a counciltion with one another of no less than 0.66(- row). Thus by this rode of reckoning, the real correspondence became

A 3X 3X 0.66 - 0.38 - onse more approximately 1,

so that this way also the correspondence adv need from 0.38 to absolute completeness.

If more than two independent series of observations are available, we may acquire additional suffence of trying the effect of partial amalgamition. Instead of throwing all our obtained values together, we may form a set of smaller combinations for each of the two compered characteristics, and then see the dean unrrelation between one set and the other. In our obove instance instead of onemarily considering p'1 P'2 P'3; we can have pt pt pt and p' p and find out their mean correlation with similar values for q. This works out autually to 0.35. Hence

12 X 2 X 0.55 - 0.35 = . approximately 1. 4/27/2-1

Thus egain, by this third may, where both t run are the mean of 9 cheerved correlational values, the correspondence once more rises from the apparent 0.38 to the real 1. (1)

3. Limits of Arsociative Problems. We have seen that "the length of the tru is said to be correlated with that of the leg, because a person with a long and hes usually a long ler and conversely;" also that this correlation is defined mathematically by any constant which deter-

⁽¹⁾ The exactness of the coincidence etween the two methods of correction is in the above instance neither greater nor less than generily cocurs in practice. It was singled cut, in order to show that the formulae still hold perfectly good even for such an enormous rise as from 0.38 to 1. The posibility of such a rise is due to the unusual conditions of the experient in question, whereby the three observations of the same objective series presented the extraordinarily small intercorrelation of Q.22.

Mines the function of any definite size of arm to the mean of the sizes of the corresponding legs. These terms, taken literally, are very wide reaching and express what we will call the "universal" correlation between the two organs.

But evidently not the most painstaking investigation can possibly secure any adequately representative sample for such universal correlations, even in the simple case of arms and legs. To begin with, they would have to be equally derived from every stage of growth, including the prenatal period; since this is the most influencial of all causes of variation in size. In the next place, they would have to come from every historical epoch, containing their fair proportion of big Cro-Magnons, little Fubfcozers, etc. Further, they must impartially include every living race, from the great Patagonians to the diminutive M'Kabbas; also every social class, from the tall aristocrats to the undersized slummers.

Practically, then, the universal correlation, even if desirable, is quite inaccessible. We are forced to successively introduce a large number of restrictions: the sample is confined to adults, to mcderns, to some particular country, etc., etc. In a word, we are obliged to deal with a special correlation.

When we proceed to more narrowly considered these restrictions, it soon becomes clear that they are far from being really detrimental. For every serious investigation will be found to be directed, however vaguely and unconsciously, by some hypothesis as to the causes both of the correspondence and of the digression therefrom. (see page 74). This hypothesis will determine a particular system of restriction, such as to set the correspondence in the most significant relief.

But from these restrictions will at the seme time proceed several kinds of grave errors. In the first place, since the restrictions are not explicitly recognized, they often are not carried out in a manner scientifically profitable; they then, the result, however true, may nevertheless be trivial and unsuggestive. For instance, a series of experiments was recently executed by one of our best known psychologists and ended - to his apparent satisfaction - in showing that some children's school-order was largely, correlated with their height, weight and strength. As, however, no step had been taken to exclude the variations due to difference of age, the only reasonable conclusion seemed to be that as children grow olier they turned out in fact to probably be the true and sufficient one.

The next fault to be feared is equivocality. For even if the controlling underthought be good, yet its indistinctness in the mind of the experimenter caused the restriction to be carried out so unsystematically, that the results inevitable have become ambiguous and fruitless.

The last is that, even with the clearest purpose, this apecialization of the correlation is an exceedingly difficult matter to execute successfully/ Only by a profound knowledge of the many factors involved, can we at all adequately exclude these irrelevant to our main intention.

Now, all such elements in a correlation as are goreigh to the investigator's explicit or implicit purpose will, like the attuating errors, constitute imputities in it and will quantitatively falsify its appraent amount. This will chiefly happen in two ways.

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ion, if our termines of the common as recommondation of the formation of the formation of the formation of the common terminal common that the common terminal common of the common of t

4. "Constriction"and"Dilation."

Any correlation of either of the considered characteristics will have been admitted irrelevantly, if it has supervened irrespectively of the original definition of the correspondence to be investigated. The variations are thereby illegitimately constrained to follow some irrelevant direction so that (as in the case of Attenuation) they no longer possess full amplityde of possible correlation in the investigated direction; the maximum instead of being I will be only a fraction, and all the lesser degrees of correspondence will be similarly affected; such a falsification may be called "constriction." Much more rarely, the converse or "dilation" will occur, by correlations being irrelevantly excluded. The disturbance i measureable by the following relation:

$$\frac{r_{pq}}{\sqrt{1 - r^2 pv}}$$

where r pg = the apparent correlation of p and q. the two variables to be compared,

rpv = the correlation of one of the above variables with a third and irrelevantly admitted variable v.

and rpq = the real correlation between p and q after compensating for the ligitimate influence of v.

Should any further irrelevant correlation, say rpm, be admitted, then

In the reverse case of "dilation,"

$$r_{pq} = r^{1}_{pq} \sqrt{1 - r^{2}_{pq}} - r^{2}_{pq}$$

these formulae will be easily senn to be at once derivable from the relations stated on pages 74 and 75. Small, irrelevant variations evidently do not affect the result in any sensible degree, while large ones are capable of revolutionizing it.

The following is an actual illustration of this constriction. I was investigating the correspondence between on the one hand intelligence of school lesson and on the other the faculty of discrimination of musical pitch. The correlation proved to be 0.49. But, upon inquiry, it turned out that more than half of the children took lessons in music and there fore enjoyed artificial training as regards pitch; here, then, was a powerful cause of variation additional and quite irrelevant the research, which dealt with the correspondence between the two natural faculties. When this disturvant had once been ditected, there was no difficulty in climinating it influence by the above formula; the correspondence between pitch and discrimination and music lessons was measured at 0.61; so that the true required correlation became

4. "Constriction" and lation."

Any conveletion of athless of althous of the consideration of the original been admitted for her her her her here expected for expective, of he original definition of the certae point has he invertible for the certae point has the invertible of the certae of the first has been at the first has been at the first has been at the first of the certae of the first has been at the first has been at the first has been at the first of the certae of the first at the certae of the first at the first of the certae pondence of the certae of the first of the certae of the first of the certae of t

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In this particular case, the more desirable course was open of elimination the constriction, practically, by confining the experiment to those children who were learning music and trorefore were on a sufficient equality as regards the training. The correlation then gained in this purely empirical way exactly coincided with the former results, being again 0.62.

5. "Distortion."

Whereas Attention and Constriction have wholly tended to reduce the apparent correlation, and Dilation to enlarge it, we now come to a third kind of impurity that may equally well reduce or enlarge. Its effects is thus amalogous to the first consequence of accidental errors discussed in the first part of this article, but unlike the latter, this Distortion does not in the least tend to eliminate itself in the longest series of observations.

Distortion occurs whenever the two sories of to be compared together both correspond to any appreciable degree with the same third irrevent variant. In this case, the relation is given by

$$\mathbf{r}_{pq} = \frac{\mathbf{r}^{1}}{\sqrt{1 - 1^{2}pv}} \cdot \frac{\mathbf{r}_{qv}}{\sqrt{1 - \mathbf{r}^{2}qv}}$$
(1)

where r pq = the apparent correlation between p and q, the two characte istics to be compared,

rpv and rqv = the correlations of pend q with some third and perturbing variable v.

and rpq = the required real correlation between p and q after compensating for the illigitimate influence of v.

Should the common correspondence with v have been irrelevantly excluded instead of admitted, the relation becomes

$$r_{pq} = r^1 pq \cdot \sqrt{(1 - r^2 pv)(1 - r^2 qv) + r_{pv} \cdot r_{qv}}$$

In the course of the same investigation above alluded to; but in another school, the correlation between school intelligence and discrimination of pitch turned out to be - 0.25, so that apparently not the cleverer but the stupider children could discriminate best! But now it was observed that a superiority in discrimination had been shown by the older children, amounting to a correlation of 0.55; while, for a then unknown reason, the schoolmaster's estimate of intelligence had shoen a very marked (though unconscious) partiality for the younger ones, amounting to a correlation of 0.65. Hence the true correlation reckoned cut to

$$\frac{-0.25 - 0.55 \times (-0.65)}{\sqrt{(1 - 0.55^2) (1 - -0.65)^2}}$$

= +0.17. This latter low but direct correlation was - under the particular l. This same formular has already been strived at, through along a very different route, by Yule. See Proc. R.S.L. Vol. IX.

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circumstances of the experiment - unquestionably about correct; so that the one originally observed of = -0.25 would have been entirely misleading.

6. Criticism of Prevelent Working Methods.

So far, cur illustration of systematic deviation has been confined to instances taken from personal experience. But it might perhaps be thought that other workers avoid such perversions of fact by the simpler method of common sense. Unforutnately, such does not seem to have been at all the case; not once, to the best of my knowledge, has any partial association between two psychological events been determined in such a way as to present any good evidential value - these are strong terms, but I think, hardly exaggerated.

Psychologists, with scarcely an exception, never seem to have become acquainted with the brilliant work being carried on since 1886 by the galton-Pearson school. The consequence has been that they do not even attain to the first fundamental requisite of correlation, namely, a presise quantitative expression. Many have, indeed, taken great pains in the matter and have constructed arrays of complicated numerical tables; but when we succeed in orienting ourselves in the somewhat bewildering assemblage of figures, we generally find that they have omitted precisely the few facts which are essential, so that we cannot even work out the correlation for ourselves.

This lack of quantitaive expression entails fat more than merely diminished exactitude. For in consequence, the expreimenters have been unable to estimate their own results at all carrectly, some have believed themselves to demonstrate an entire absence of correspondence, when the latter has really been quite considerable; whereas others have presented to the public as a high correlation what has really been very small and often well within the limits of mere accidental coincidence; these limits they have had no me as of determining, and moreover their data were usually obtained in such a way as to make it unnecessarily large.

Seeing, thus, that even the elementary requirements of good correlation work described in the first part of this article have been so generally deficient we cannot be surprised to find that the moreaddinced refinements of procedure discussed in the second part have been almost wholly unregarded; so that the final results are saturated and falsified with every description of impurity. In this respect, unfortunately, it is no longer possible to hold up even the Galton-Perrson school as a model to be imitated. The latter must now perform the very different office of saving us from detailed criticism of inferior work, by enabling us to form an opinion as to how Intfar the defect permeates and vitiates even the best existent correlational research.

As example, we will take Pearson's chief line of investigation, Collateral He edity, at that point where it comes into closest contact with our own topic, Psychology. Since 1989 he has, with government sanction and assistance, been collecting a vast number of data as to the amount of correspondence existing between brothers. A perliminary calculation, based in each case upon 800 to 1,000 pairs, led, in 1910 to the publication of the following momentous results:

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Coefficients of Collaboral Heredity. Collelation of Pairs of Brothers.

Physical	Characte	1.3.	Mental Charact	ers.
(Family Monsurements)		(School Observations)		
Stature Foresim Span Lyc-colour		0.5107 0.4912 0.5494 0.5169	Intelligence Yivacity Conscientiousness Popularity Temper	0.4559 0.4702 0.5329 0.5044 0.5068
(School Observations)		ations)	Self-consciousness	0.5915
Cophelic ind Hair Colour Health		0.4861 0.5452 0.5303	Shynoss	0.5281
	lieen	0.5171	Mean	0.5214

Dealing with the means for physical and mental characters, we are forced to the ipadiant the categories in the categories and moral nature is, quite as such as out physical nature, the cuteme of heredity factors,

Mow let us consider how these coefficients of correlation will be affected by our "systematic deviations." To begin with there is the "Attenuation" by errors; since it evidently cannot be assumed that the schoolmasters' judgments as to conscientiousness, temper, etc. are absolutely infallible. On page 90, it has been shown that deviation from this source may be estimated by the following do male:

$$r_{pq} = \sqrt{r_{p^{\dagger}p^{\dagger}} \cdot r_{q^{\dagger}p^{\dagger}}}$$

To ascertain r pipt and r high, I am aware of no precise data haimson beyond

that found in some experiments of my own, where the independent intellectual gradings for the same series of subjects correlated with one another on an average to the amount of -.64. As an other occasions very competent persons have estimated this to be as much as should be expected, as as intellegence is about the most easily gradable of all the mental qualities mentioned by Pearson, to there is so for no reason to suppose that his "great number of masters and mistresses" did on the whole any better. Hence, even if we could assume that the mistakes in estimating one brother were independent of the mistakes in estimating the other, then the true correlation rould be about, not 0.5172,

but 0.5172 = 0.81, an extent of difference that seriously $\sqrt{6.64}$ X 0.64

modifies our impression of exactitude from all these coefficients to four places of decimals. When we further consider that each of these physical and

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ម្រុក ប្រជាជាស្ថាល ប្រាក្សា មាន នៅ ប្រជាជាស្ថាល ស្រុក ស ស្រុក ពីស្រុក ស្រុក mental heredity can hardly be more than mere accidental chincidence.

Let us next proceed to irrelevant correlation, and take for our theme postnatal accidents connected on the one side with brotherhood and on the other with the mental qualities. Pearson's primary intention seems to have been to make his correlations as "universal" as possible and in one place he expressly mentions that education is smeng the causes contributory to variation. Hence, he is more than consistent, in that he forms his correlation without regard to the fact that the correspondence between the brothers' "conscientiousness." "popularity," etc., must be in great measure due to their coming under the same home influences. But such a correlation can scarcely be accepted as scientifically valuable. Fo we do not really know anything precise about the assimilating effects of heredity, when our observed correspondence is perhaps chiefly due to the brothers having the same emount of hampers and pocket-money. Still less can we, then, fairly compare such results with that obtained from physical measurements, where common home life has little or ne effect. The factor of post-natal accidents, therefore, cannot but be regarded as irrelevant, and consequently the coefficients of correlation must be taken as hopelessly "distorted."

But even consistence cannot be upheld throughout the matter. For though the effect of postnatal life has thus been admitted with regard to education at home, it has perforce been excluded as regards public education. For only those brothers have been compared together who are at the same school; the coefficients of correlation would certainly diminish if those also could be included who are living in a totally different manner, have gone to sea, etc., The correlations are therefore also illegitimately "dilated."

If this work of Pearson has thus been singled out for criticism. it is certainly from no desire to undervalue it. The above and any other systematic errors are exentually capable of adequate elimination, and this article has itself, it is hoped been of some use towards that purpose. Such correction will no doubt necessitate an immense amount of further investigation and labour, but in the end his results will accuire all their proper validity. My present object is only to gu rd against premature conclusions and to point out the urgent need as still further improving the existing methodics of correlation work, a mothod of investigation which he mimself has so largely helped to create and by means of which be is carrying light into immense regions hitherto buired inthe obscurity of irresponsible speculation. The fundamental difference between his prodedure and that here racommended, is that he seeks large natural samples of any existing series sufficiently homogeneous to be treated mathematically; whereas here smaller samples are deemed sufficient, but they are required to be artificially selected, ordered, and corrected into full scientific significance. His methods are these of pure statistics these inculcated here may be more aptly termed "statisticoids."

7. Number of Cases Desirable for an Experiment.

This leads us to the important question, as to how many cases it it advisable to collect for a single sories of experiments. In actual practice, the greatest diversity has been apparent in this respect; many have thought to sufficiently establish important correlations with less than ten experimental subjects while others have thought it necessary to gather together at least over a thousand.

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if it is a second to the control of - Lednoch was no in a said for right length in the last file in the last the นอทา จำลองได้ได้ ทองต้องเครี่ เซอส์ชั้น อรี่ รู้เครียกการ เลื่อง ค.ศ. ค.ศ. อ. . สาราร์โลส สสัตย์ได้มีผ . DEWILL AND A Now, a series of experiments is a very limited extract, whose disposition is, nevertheless, to be accepted as a fiar sample of the whole imment remainder. Other things equal, then, the larger the sample the greater its evidential value and the less chance of a mere occassional coincidence being mistaken for the permanent universal tendency.

This danger of accidental deviation has been discussed in the first part and there shown to be strictly measurable by the "probable error." We there saw, aloo, that this danger can hever be entirely eliminated by any sample however large, so that it is necessary to accept some standard less rigorous than absolute certainty as sufficient for all practical purposes; usually, the danger of ners chance coincidence is considered to be inapprociable when a correlation is observed as much as five times greater than the probable error, seeing that more chance would not produce this once in a thousand times. Honce, evidently, the accidental deviation depends, not only on the number of cases, but also on the largeness of the really existing correspondence; the more perfect the latter, the fewer the cases that will be required to demonstrate it conclusively; and this tendency is augmented by the fact that the probable error, besides varying inversely with "n", does so to a further extent with "r" (see formula). It was shown in the same part that the size of the probable error also varies accordingly to the method of calculation- and to such an attent that twenty cases treated in one of the ways described furnishes as much certitude as 180 in another more usual way. If the common trifold classification be adopted, an even appeater number is required to effect the same purpose; and if the correlation be not calculated quantitatively at all, but instead be presented in the customary fashion to the reader's general impression, then no number of cases whatever apprear sufficient to give reasonable guarantee of moof.

While this the number of subjects is not by any means the sole factor in diminishing even the accidental deviation it has no effect whatever upon the far more formidable systematic de intion except that it indirectly leads to an enormous augmentation thereof. When we are taking great pains to be able to show upon paper an imposing number of cases and a distinutive probable error, we are in the self same provess most likely introducing a systematic deviation twenty times greater.

From this we may gather that the number of cases should be determined by the simple principle, that the measurements to be aggregated togother should have their error brought to the same general order of magnitude. An astronomical chronometer, with opring-detent excapement, is not the best travelling clock; nor is there any real advantage in gracing upon a milestone (as has actually been done by an infatuated mathematicien!) the distance to the nearest village in metres to three decimal places. Now, the present stage of Correlational Psychology is one of pioneering; and, instead of a few unwieldy experiments, we require a large number of small ones carefully carried out under varied and well considered conditions. At the same time, however, the probable error must be kept down to limits at any rate small enough for the perticular object of investigation to be proved. For such a purpose a probable error may at present be admitted without much hasitation up to about 0.05; so that, by adopting the method of calculation recommended, two to three dozon subjects should be sufficient for most purposes. The procision can always be augmented subsequently, by carrying our similar experiments under similar conditions and then taking averages. Only after a long preliminary exploration of this rougher sort, shall we by in a position to effectually utilize experiments designed and executed from the very beginning on a vast scale.

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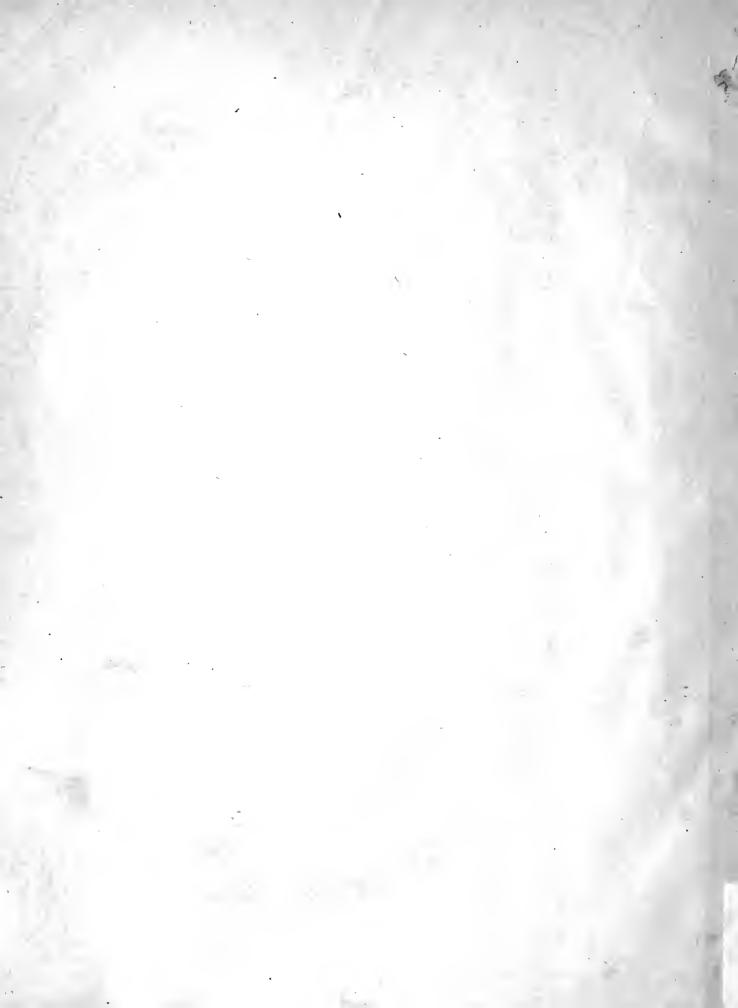
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